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Earth System Dynamics Discussions

Interactive comment on "Hydrological cycle over south and southeast Asian river basins as simulated by PCMDI/CMIP3 experiments" *by* S. Hasson et al.

Anonymous Referee #2

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Manuscript: esdd-4-109-2012: Hydrological cycle over south and southeast Asian river basins as simulated by PCMDI/CMIP3 experiments

Major remarks

The authors investigate the simulated hydrological cycle over four major basins in the South Asian monsoon region (Indus, Ganges, Brahmaputra and Mekong). Here, they consider current climate simulations and future projections (A1B scenario) obtained from an ensemble of CMIP3 GCMs. Generally, a catchment focused evaluation and consideration of projected changes of the hydrological cycle is a valuable approach. Also, choosing the South Asian monsoon region is justified as this is one of the hot

spot regions of the globe.

Unfortunately it does not become clear what is really new in this study. First, I wonder why CMIP3 models are investigated, and not CMIP5 models. CMIP3 models are designated as 'present day climate models', which is actually not correct anymore as this is now true for the CMIP5 models. Many results on the global hydrological cycle and its projected changes simulated by the CMIP3 models are already published and, e.g., summarized in AR4 of IPCC (Solomon et al. 2007). For example, Dai (2006) considered various precipitation characteristics in 18 CMIP3 models using global maps, Milly et al. (2005) considered future changes in runoff projected by CMIP3 models. The same also applies to studies of South Asian monsoon precipitation simulated by the CMIP3 models (e.g. Annamalai et al. 2007). Well, in this respect, the thorough analysis and discussion of reasons for common CMIP3 model biases over the South Asian region would still provide some new insights, but this is not done in the present study.

References: Dai, A., 2006: Precipitation Characteristics in Eighteen Coupled Climate Models. J. Climate, 19, 4605–4630. Milly, P.C.D., K.A. Dunne, and A.V. Vecchia (2005) Global pattern of trends in streamflow and water availability in a changing climate. Nature 438, 347–350.

On p. 121 line 1-2, it is proposed 'to understand the range of climate projections in the later part of the XXI and XXII centuries', but I couldn't find explanations with regard to this objective. Sometimes the authors state a kind of ranking of model behaviour between the basins (e.g. p. 133 – line 14-17). What's the scientific value of such a ranking? The reasons of the different model behaviours are of interest, but no explanation is given here. Differences in the precipitation characteristics are explained by different climatic conditions (p142 line 6++), but this is known fact and not related to the models. Certainly the models should simulate the different regimes. Biases in hydrological variables are explained by biases in precipitation, but a general explanation for the latter is not given, nor for the large inter-model ranges, with one exception.

On P143 – line 16++. Here, some monsoon features are explained, but unfortunately I don't get the connection with the intermodel spread and the model biases from the current text.

The present study is merely descriptive, and it very explicitly describes the simulation characteristics for each individual CMIP3 model for each river basin. In addition, it provides a lot figures that look all very similar. The authors need to find a way to summarize and present their results in more condensed form. I suggest focusing on important results and summarizing the important results for each basin in a concise way. The authors should avoid the description of every single model behaviour (science is not a beauty contest). Except for considering specific outliers of interest, they should use summarizing, more general descriptions such as the ensemble mean, ensemble spread and some measure of clustering. With respect to the latter, the authors provide a very interesting result that gets almost lost in the current version of the paper (see Sect. 5.1). They note that the ensemble means over the Brahmaputra and Mekong basins do not resemble any single model result. This may mean that the ensemble mean does not represent a realistic solution of climate over this region. In this respect, defining a measure that may indicate this failure of an ensemble mean would be a large step forward. In this respect, it is valuable to note, that a n umber of models clusters for specific basins and variables, but usually not which models exactly agree with each other (such as done, e.g., p. 132 – line 13-15).

The use of some hydrological variables is not common or does not seem to provide new insights. The authors use the term water balance (starting already in the abstract) to quantify P-E, while the correct hydrological usage of this term is P-E-R = dW/Dt. For long-term climatological means (dW/dt $\rightarrow$ 0), this becomes P-E-R = 0. If this equation is valid for an individual model its water balance is closed. (This is also investigated in Sect. 4.1.3, but using the misleading terminology.) In addition the strength of the hydrological cycle, P+E, is considered. But all biases or changes in this quantity are related to biases or changes in precipitation and evaporation that are also considered. In

this way, considering P and E separately is sufficient, and considering the constructed quantity of P+E is obsolete and does not gain any valuable additional information.

Some of the analyses for the current climate seem to ignore knowledge about the basins itself. In this respect it is very inappropriate to compare the simulated runoff over the Indus basin to observed Indus river discharge. The latter is strongly anthropogenically influenced due to the large irrigation activities over the Indus basin (Karim and Veizer 2002). This means that large amounts of water are removed from the Indus River and they are evaporated over the irrigated areas. Consequently, as none of the CMIP3 GCMs includes irrigation, it cannot be expected that any of the GCMs is simulating the observed discharge values. Thus, it is not surprising that almost all GCMs overestimate P-E = R over this region.

For the other three basins the behaviour of P is dominant (which is also correctly stated by the authors) and strongly determines the characteristic of the other hydrological variables (E, R). But as stated above, CMIP3 model precipitation has been investigated in previous studies for current climate and future projections, so that again the questions arises which results of the present study provide new insights. For the Mekong an additional problem occurs due to the relatively narrow structure in the Northern part of the basin. Here, the coarse scale resolution GCMs certainly have problems to adequately simulate the fine-scale precipitation over this area. This reason for biases is neglected in the present study.

The English needs major improvements. For future versions of the manuscript, I suggest proof reading by a native speaker.

In summary, major revisions are necessary before the paper may be recommended for publication in Earth System Dynamics.

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